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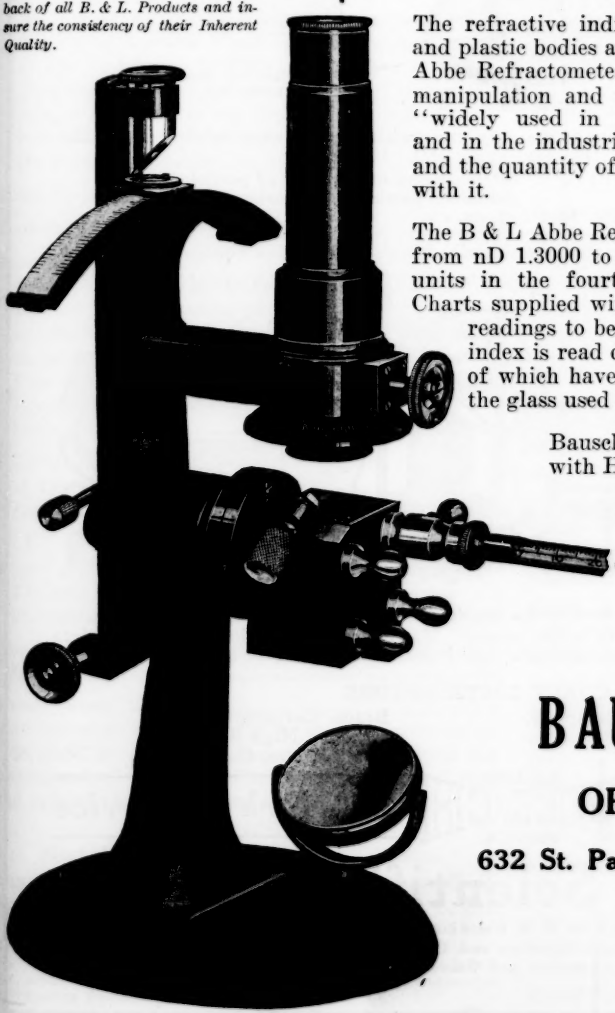

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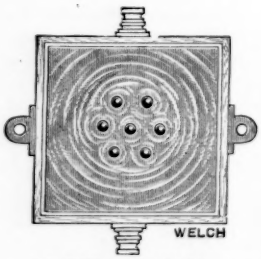

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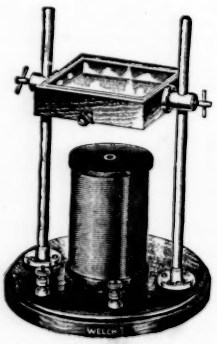

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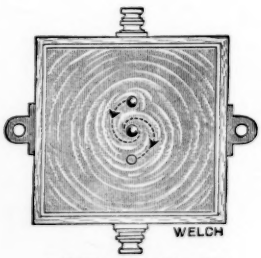

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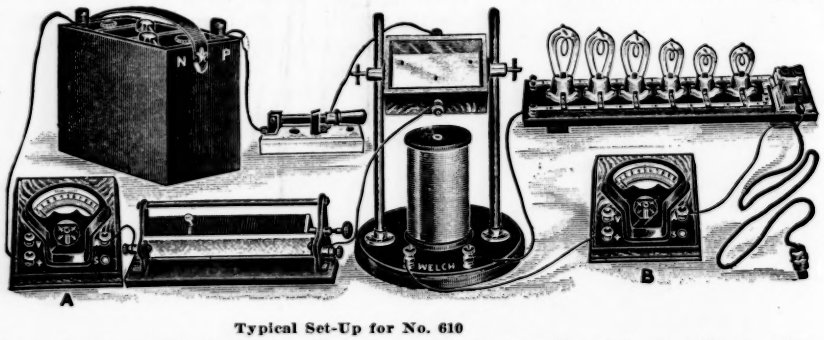

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WHAT IS STATISTICS?[1]

WHEN one assembles the phrase: "What is Statistics?" he must expect first to defend his grammar. Should he not ask: "What are statistics?" We do not say "What are Mathematics?" or "What are Kinematics?" Statistics in the original and plural sense means collections of data, first data of the state and by derivation any kind of data. In this sense a statistician is any person who collects such material or who discusses it by any method or without method. It is as though by mathematics we meant collections of mathematical tables or formularies and by a mathematician one who gets the material together. Such usage is justified by custom for statistics and statisticians but not for mathematics or mathematicians. It represents a wide difference of attitude toward the two subjects and the workers in them.

Another difference is this. We see many college books appearing with titles such as "Elements of Statistical Methods," written chiefly by economists. We do not find the book catalogs flooded with texts on "Elements of Mathematical Method" or introduction to biological method. Why? May it not be that mathematicians and biologists, though interested in the technique of their fields of study and instruction, regard those subjects primarily as sciences in which the more important element is really the scientific viewpoint, the principles of the science in question, whereas teachers and writers of texts on statistical method believe that their subject is not a science but a technique? Which group is the wiser, or are mathematics and biology on the one hand and statistics on the other such contrasting entities that each group is wise in its own conceits?

If a great institution like the Rockefeller Institute for Medical Research desires to make outstanding contributions to biology it perforce incorporates with its staff one or more outstanding biologists. It does not consider adequate the purchase of a few mediocre texts on biological technique, to which a staff not especially trained in biology may turn to find the biological method and apply them in its work of advancing medical science. In such institutions we find also trained chemists, not merely reference texts to chemical methods. And this even though every investigator in medical science has a not inconsiderable training in both chemistry and biology. Despite such training, and perhaps because of it, he does not trust himself unaided to extract from the manuals of those

[1] Read at the Rockefeller Institute, April 8, 1927.

sciences and to apply the methods which he needs in his work.

Yet in the past and perhaps even to-day many leading institutions do seem to consider that the statistician is not necessary to them for the discussion of their statistical problems and appear content to have the members of their staff dip into some statistical manual, extract what they may of some ill-comprehended statistical method or technique and believe or hope themselves competent to elicit the proper inferences and avoid the improper ones by the routine application to their data of these miraculous extracts. That is why I ask: What is statistics, not what are statistics, nor yet what is statistical method? And I venture to suggest in a tentative, undogmatic sort of way that it is largely because of lack of knowledge of what statistics is that the person untrained in it trusts himself with a tool quite as dangerous as any he may pick out from the whole armamentarium of scientific methodology.

By the use of statistics in the singular, like mathematics or kinetics or physics, I intend precisely to direct the emphasis toward the principles of the science, toward its significance for human thought, toward its meaning. And I shall ask you to think of statistics as dealing with realities like physics, not merely as a refined system of logic like modern abstract mathematics. Statistics has its abstract side in the theory of probability, just as physics has in its higher mathematical reaches. But as the significance of physics can be well discussed with a very simple mathematical technique, so statistics may be understood without any of the higher branches of the theory of probability, without any mathematics beyond precollegiate algebra, but it can not be understood without thought.

Thinking is apparently a painful process. We do not naturally like even to ponder our definitions. Some of us seem but dimly to recognize that there are such things as definitions. We use words in a vague sense by habit and often in some ignorance of their denotations and in complete ignorance of their connotations. Yet not statistics nor any other science can replace or even exist without the science of language and of logic. Newspaper headlines are not a safe guide to a scientific glossary. You pick up a paper and read: "December wetter than usual." What does that mean? What is the true significance of the word usual? Should not the headliner have written "December wetter than the average"? What is the difference between the usual and the average? What is meant by an average, anyhow? This simple headline contains a deal of statistics. Again we read: "Coolidge likely to run for a third term." What does third mean? And likely—this seems a statement relative to probability, or did the writer really mean liable?

Next in importance to an appreciation of the meaning of words and of the significance of ordinary logical processes, the statistician should place the understanding of the simple notions of probability. The reason for this is that he desires to pass from the particular to the general. Now a general or universal proposition may be postulated but can never be completely proved. This means that unless we take a universal as a tenet of faith we can consider it as established only to a presumably high degree of probability and only within certain more or less restricted limits. If we say that the specific gravity of urine is 1.020 we make a universal statement, but it is not true. If we say that the specific gravity of urine lies between 1.015 and 1.025 we again make a universal statement, which again is not true in fact, though it is less false. The reason we need the knowledge of probability is to have a better way to state our universals or generalizations and a more precise understanding of what they mean.

To persons brought up as most of us are to take our knowledge chiefly on authority rather than to get it by inquiry it is at first a great shock to learn that a statement involving probabilities is more true and more precise, in brief more significant, than a categorical statement. But without such an understanding we are unable as scientists, other than as scientific cataloguers to commence to make answer to Pilate's question: What is truth? I may illustrate by an imagined example. Suppose there is an infection among a population of animals. The incidence is partial. We wish experimentally to determine what that incidence is. How do we proceed?

First, we may conceivably test all the animals. If we do this we come to a definite statement of the percentage affected (errors and omissions of observation excepted). The result is a particular fact, it is not a generalization. As we have used up, by hypothesis, our total universe of animals there is no need of generalization. But if the universe is large it would be impracticable to test all the animals. We must resort to sampling. Suppose we select at random a dozen animals and find eight infected and four free of infection. Then our particular is just that, namely, of that sample of twelve there are eight infected and four not. Shall we pass to the conclusion that two thirds of the animals are infected? Assuredly not. We could not expect all samples to run exactly alike. Suppose that we make the tentative assumption that the incidence is exactly two thirds in the total population. What would be the results of repeated selection and observation of random samples of twelve?

We are back to the simplest type of probability-problem. If $p = 2/3$ be the chance of infection and $q = 1/3$ be the chance of freedom therefrom, then $p \cdot p = p^2$ will be the chance of infection of both of two animals A and B, $p \cdot q$ the chance that A is infected and B is free, $q \cdot p$ the chance that A is free but B is infected, $q \cdot q = q^2$ the chance that both are free. The analysis may be made for twelve instead of two. There are thirteen possibilities; all twelve may be infected, eleven may be infected and one free, ten infected and two free, and so on down to none infected but all free. Each of these possibilities has its own probability. The respective values are:

Selection	Chance	Chance	Chance	Relative
12– 0	p^{12}	4096/531441	.0077	.03
11– 1	$12p^{11}q$	24576/531441	.0462	.20
10– 2	$66p^{10}q^2$	67584/531441	.1272	.53
9– 3	$220p^9q^3$	112640/531441	.2120	.89
8– 4	$495p^8q^4$	126720/531441	.2384	1.00
7– 5	$792p^7q^5$	101376/531441	.1908	.80
6– 6	$924p^6q^6$	59136/531441	.1113	.47
5– 7	$792p^5q^7$	25344/531441	.0477	.20
4– 8	$495p^4q^8$	7920/531441	.0149	.06
3– 9	$220p^3q^9$	1760/531441	.0033	.01
2–10	$66p^2q^{10}$	264/531441	.0005	.00
1–11	$12pq^{11}$	24/531441	.0000	.00
0–12	q^{12}	1/531441	.0000	.00

The details of the calculation may appear complicated, but the results at least are simple and typical. The actual chance of finding two thirds infected in a sample of twelve taken from a universe in which the chance of infection is two thirds is only .2384, less than one in four. You have three chances in four of not realizing in any particular sample the proportion in the universe; your chances are almost as good to realize a division 9–3 or 7–5 as 8–4, and you have about half as good chance to realize either 10–2 or 6–6 in the sample. With correspondingly harder luck you may get complexions in the sample still further from that in the universe.

Clearly this hypothetical case may be reversed. Instead of postulating a division in the universe with twice as many infected as free, and calculating the chances of samples of twelve of different complexions, you may admit the observation of eight infected and four free in the sample and ask what may reasonably be the proportions in the universe if this observation be not too unreasonably rare? The answer requires the solution of a quadratic equation, a problem in precollegiate algebra, and the formula for the mean error (standard deviation) of sampling $\sigma_p = \sqrt{pq/n}$, which may be proved by elementary algebra. The work will not be given here, the result is that there is a probability of about 1/2 that the proportion in the universe will lie between 16/28 and 21/28, *i. e.*, between $4/7 = .57$ and $3/4 = .75$ and a probability of about 2/3 that the proportion of the universe lies between .52 and .78.

This type of statement of the inference assumes that the experience realized in the sample is not too anomalous. The inference might better be put thus: If the proportion of infection in the universe lie between .52 and .78 then the experience $2/3 = .667$ in a random sample of twelve is not so unusual as to be realized less than twice out of three trials, or if the incidence of infection in the universe is less than 52 per cent. or more than 78 per cent., then realization of a random sample of twelve will be rarer than one chance in three. The enigmatical little word random deserves attention. We mean that the sample of twelve is merely picked out by chance. There is nothing to prevent any particular random sample from having a very rare complexion. In fact the very idea of randomness implies that once in a while the sample will be far from typical. One does have hard luck. When one generalizes from a sample he simply has to assume that his luck has not been too bad, that his sample is really fairly typical.

Now in actual life sampling is not an entirely random process. Suppose we are dealing with plague-infested rats and want to estimate how widespread their plague is. We go out and get twelve rats. How do we know that it may not be far easier to get sick rats than well rats or contrariwise far easier to get well rats than sick ones? We do not know. The processes of getting the sample may involve systematic errors. Quite in addition to the difficulties of ensuring randomness. The sample may accidentally be atypical or it may be systematically unsound by virtue of the physical limitations imposed by realizing the sample. What any statistician has to affirm before he may draw his inference on the universe is that his sample is neither atypical nor unsound. No amount of theory will ensure this, but only an adequate experience or sense of the eternal fitness of things whereby he may judge that in

fact he has a fair randomness in his method of selection and has not had hard luck in the selection.

I have spoken of the necessity of knowing the meaning of words and appreciating the value of logic. Then, next, of the importance of understanding the elements of the theory of probability and realizing the ways in which chance may fortuitously or sampling may systematically mislead. I will give a further illustration of a probability calculation. Suppose that we are sampling plague-infested rats and pick out twenty-four rats. Let us turn them over to a technician to examine and assume that for some reason or none the technician examines them by twos instead of singly, and finds eight of the twelve sets of two infected, but four free. What is the inference? As four pairs are free, at least eight individual rats are free. As eight pairs are infected, at least eight single rats must have been infected. The incidence must be between one-third and two-thirds inclusive in this particular sample of twenty-four. Can we say more? Let q be the chance for freedom of infection in the universe. Then q^2 is the chance that both of a pair be infected. The experience gives $q^2 = 1/3$ or $q = .58$ and $p = .42$. The probability is that in this sample the infectivity which must lie between one-third and two-thirds is actually much nearer the former figure, namely, $p = .42$, and that ten of the twenty-four rats were infected. We may further superpose on such a calculation as this a discussion of variations of sampling, as above, and thus elicit the maximum information with respect to the constitution of the universe.

But there is one other consideration of very great importance with which the statistician should be familiar, namely, the analysis of association of attributes. We are always asking about the connection of this defect with that. Do men tend more than women to schizophrenia? Do cardiac and renal disorders tend to come together? Does like tend to marry like? Are there anthropologic types which run to certain disorders? These are more complex universes than the simple universe of infection or freedom from it. We have here a double dichotomy or an even more complicated situation. Let us take the usual case, the simplest, of a universe of double characters A and B where for each individual of the universe we may say that he has the character A or fails to have it and has the character B or fails to have it. There are now four possible kinds of individuals in our universe.

1° Those with both characters A and B, say (AB)
2° Those with A but not B, say (Aβ)
3° Those with B but not A, say (αB)
4° Those with neither A nor B, say (αβ)

The logical analysis of the universe can not be made, because the constitution of the universe can not even be specified, without a knowledge of four numbers, *i. e.*, the numbers in each of the four classes of the doubly dichotomized universe.

Time and time again people bring me material for statistical discussion where no discussion is possible but to point out that they have not all four of the numbers necessary to determine an association; often they have three of them, sometimes only two, and once in a while only one. No amount of statistical technique will close this logical hiatus. A vast amount of material avails little if it be not of the sort needed. A statistical clerk may assemble data, but a real statistician, well in advance of making any statistical analysis, must discern what data will be necessary to suit his inferential purposes. Any one can make a survey, but few seem able to lay out one that will be fruitful. Hindsight is better than foresight; it is not easy in advance to bethink oneself of what he may need. Thinking, like loving, is a painful thrill. I could extend the discussion of association with illustrative examples and amplify it by calculations of sampling errors to determine whether or not, in the particular examples, the associations were significant; but I will content myself with referring to the treatment in Yule's "Statistics" and with the statement that most of our texts on statistics say little or nothing about this most fundamental matter.

As in my course in vital statistics at the Harvard School of Public Health, so here in my brief hour I am trying to lay the stress on basic ideas without which detailed calculations become but empty, if impressive, virtuosity. In my own work I try not to permit myself the ease of routine until the figures themselves have been critically examined. Often relatively simple methods are finally adequate, and if so, so much the better. It does not pay to build too high on doubtful facts. Some years ago I became interested in the problem of computing life tables for native and foreign-born whites in city and rural districts. The Census Bureau kindly supplied the specific rates at five-year periods. I did not like the looks of the material. Consider it for a moment. The rural native white males have lower death-rates at all ages than those in the city until we reach the tenth decade where the figures become somewhat erratic, due perhaps in part to small numbers and in part to poor age reporting. Likewise the foreign whites do better in rural than city districts. These observations are both as might be expected. But the foreign city whites under forty-five years of age have lower death-rates than the native whites; whereas the rural foreign whites have higher rates than the rural native whites. The facts as supplied are prob-

ably correct, but are they significant for a life table calculation? What is a life table? It is a complicated mathematical function of the specific death-rates. And what is it for? It is to give the expectation of life at any age and to represent the vital condition in a stationary population.

What is the importance of the conception of a stationary population? It is to get at the biologic facts about a population independent of the actual age distribution of the population. Thus the average age at death in New York State other than New York City in 1923 was fifty-one years and the general death rate was 14.8 per 1,000. Now if the deaths just replaced the births in numbers and if each age group were just replaced by the lower age group less its deaths so that the population were stationary, then at a rate of 14.8 per 1,000 per annum it would take $1{,}000 \div 14.8 = 67\frac{1}{2}$ years on the average for a given group of births to die out. In a stationary population a death rate of 14.8 means an average age at death of $67\frac{1}{2}$ years, not 51 years as was actually the case in New York State in 1923 with its non-stationary population. Conversely, an average age at death of 51 years in a stationary population would mean a death-rate of $1{,}00 \div 51 = 19.6$ instead of 14.8. There is thus a great difference between figures in a stationary and in an unstationary population. The differences would be far more striking in New York City than in the rest of the state. Now the complete expectation of life at birth, which may be obtained from the life table, purports to be the biologic average age at death independent of population distribution and 1,000 divided by that age the true biologic death-rate, and there can be little doubt that the life table does improve on the average age of death or the general death-rate as an index of length of life.

That which I wish especially to point out is the fact that the life table does not actually give a true measure of the biologic situation. In constructing the table we use as of a certain date the specific death-rates by age. We may consider the rates which are found in the state report for New York, 1923. The age group 60 to 64 has a death-rate of 41 for the city, 31 for the rest of the state and 35 1/2 for the whole state. Except as the persons now in that group have moved into the state they have lived through in their youth the very high infant mortalities, the high child mortalities and the high mortalities of early adult life which prevailed from 1865 to 1890, times when the infant mortality was three times as high as at present, the death-rate from tuberculosis as high as three to four per thousand per annum (four times the present figures), those from diphtheria and scarlet fever over one per thousand (ten times the present figures) and typhoid fever perhaps one per thousand. If there has been a natural selection of the death-rate in man during this period, the weaklings have been weeded out and the age group 60 to 64 should be a stronger group than if its weaker members had not been so decimated in early life. On the other hand, if these diseases of early life which swept the population now in this age group and took so many of its numbers, wounded and crippled many of those that were not killed and left them with serious sequelae, the present survivors in the age group 60 to 64 may be weaker than if they had not passed through the fire. Whether the net result is in favor of the natural selection of the death-rate or opposed to it I have no idea, and in the absence of knowledge on this point we can not maintain, even though we do tacitly assume, that the death-rate of 35 1/2 in this group is compatible with the low specific death-rates which we now find in early life. What our life table calculation does is to use the age specific death-rates as though they were those in a stationary population and upon that assumption to calculate the characteristics of that stationary population. It is important to realize the existence of all these tacit assumptions even if we believe that they do not have a material effect upon the result and upon the validity of our practical conclusions.

We may now return to the specific death-rates of the rural and urban native and foreign white populations and ask whether a calculation of life tables would be *a priori* worth while. The city rates for natives are higher than for rural; the same is true for foreign. We have considerable experience to indicate that really urban death-rates are, or until very recently have been, higher than rural death-rates, and that they are higher all along the line from infancy to old age, so that the net effect of the less favorable urban environment would seem to be more crippling than selective. Furthermore, we find that the foreign rural whites do worse than the native rural whites. This does not seem unreasonable; they may have difficulties of acclimatization, whether economic or meteorologic, and they may have been subjected to less favorable conditions in earlier life. To offset this we have to recognize that our immigrants may be a favorably selected lot, more robust than the compatriots they left behind them and possibly to be expected to excel our natives with whom they are compared. When we turn to the foreign city whites in contrast to the natives we find that to the age of forty-five they do better than the natives. Is this a true biologic finding of the sort we should desire to incorporate in a stationary population or is it some accident of selection? This is the primary statistical question. So long as the specific

death-rates are let stand for what they are, any doubt with respect to the proper answer to this question remains a doubt and as such subject to discussion. Once these specific death-rates have been incorporated into a life table we have added to them a whole handful, I will not say a whole brainful, of tacit assumptions, possibly ill-realized and now easily overlooked, and we may get out of our life table more the consequences of those assumptions than those of the original biologic facts.

It seems to be a curious fact that careful logical thinking, and even the appreciation of the meaning of words or the significance of definitions, is considered to be mathematics and that very careful attention to what one means is rated as higher mathematics irrespective of whether any formulas are used. Thus if we teach a person the arithmetical routine of calculating a life table from age specific death-rates we are not forcing him to indulge in mathematics other than the most elementary, but if we ask him what he really is doing or what a life table really is and whether by doing what he is doing he is really getting what he purports to be getting, then we are submitting him to that particular cruel sort of torture known as higher mathematics. This attitude of mind was perhaps illustrated in an earlier terminology whereby the physical sciences were called natural philosophy and the biological science natural history. The latter were the story of nature; there were voyages to go, things to see and romances to relate. This was not higher mathematics and was balm to the soul. The physical sciences were, however, not the story of nature but the wisdom thereof; there was thinking to be done, analyses to make and proofs to give. This was higher mathematics and was a test of the intellect. We know better now. Physics and astronomy have become metaphysics and romance; biology has become hard-boiled. Physics consists of schizophrenic phantasy or manic ecstasy with a maximal obfuscation of complicated mathematical technique; biology means close thinking, with a minimum of formal mathematics. Statistics is higher mathematics—let it go at that—and the statistician is a fellow who has to find out within practical inferential limits something of real significance. This requires some aptitude and some training, and of course it is not easy to supply either; both must grow, and that takes time. For these reasons organizations like this are coming to realize the importance for their work of adding the statistician to the biologist and chemist.

If time still permits I should like still to amplify and to emphasize the use that a statistician may be to you in experimental medicine. I have spoken for the most part of the statistical thinking to be done on observations already made. In the brief reference to association of attributes there was mention of the difficulty of foreseeing those elements which logically must be known if we shall be put in a good position for inference. This applies also to the lay-out of an experimental program and because we do not have either the time or the resources to assemble by the experimental method the mass of material that may be collected in the general observational field, it is all the more important that we plan our work so as to get the most from the fewest experiments. As we are usually limited to small samples we have to be particularly circumspect. On this subject the best reference seems to be R. A. Fisher's "Statistical Methods for Research Workers." The title should not mislead you into feeling that Fisher is really interested in technique of a routine sort, nor divert you from concentrating your attention especially upon such trenchant paragraphs as that on page 111, where the author states: "The need for duplicate experiments is sufficiently widely recognized; it is not so widely understood that in some cases, when it is desired to place a high degree of confidence on the results, triplicate experiments will enable us to detect with confidence differences as small as one seventh of those which, with a duplicate experiment, would justify the same degree of confidence." This statement is very important. Many of you know that the precision of statistical data increases only with the square root of the number of observations, and from that a careless thinker might conclude that triplicate experiment would increase the precision only as $\sqrt{3}$ is to $\sqrt{2}$ or less than 25 per cent., whereas Fisher's statement shows that with proper planning the increase may sometimes run to several hundred per cent. He reverts to this matter on p. 224 in a section upon the technique of plot experimentation, but the problem is the same as that of experimental medicine. It is this pregnant philosophy of experimentation which is the most significant part of Fisher's book for your purposes and which you may easily overlook or fail to grasp. True meaning is always hard to grasp, but once understood it stands by long after detail has vanished.

Logic and definitions, probability and universals, association of attributes and multiple universals, real significance *versus* arithmetical operations, planning experimental procedure, these are five considerations which have to be borne in mind in the kind of work you are doing and the last is perhaps both the most difficult and the most helpful, the one which would most repay in its saving of time any expenditure you might make to understand it. May I close with the suggestion that, as you now have on your staff in your department at Princeton one who thoroughly

comprehends planning experiments, you get him some time to follow up these generalities of mine with an intensive discussion of that special statistical subject?

EDWIN BIDWELL WILSON
HARVARD SCHOOL OF PUBLIC HEALTH

WHAT SHOULD WE DO WITH A HARVEY OR A LAENNEC?[1]

DURING the greater part of the year most of the members of our society are integral parts of various medical schools. Our chief interests are devoted to academic medicine. To-day, however, we meet in a city which is far from all medical schools and our thoughts properly turn to the larger problems connected with research in clinical medicine. The American Society for Clinical Investigation is composed of clinicians who have been selected because they have shown ability in the fields of research. We have as our chief concern and as our chief responsibility the perpetuation of the research spirit in clinical medicine. Therefore it is our duty to sound a note of warning whenever we believe this spirit to be in danger.

With the tremendous growth of our medical clinics and medical schools we are in danger of being swamped in this process of expansion. There is a great demand for young men to fill the various positions in these large clinics and the demand falls chiefly on the membership of this society. Almost every man here is planning a career of academic medicine in which he hopes to be able to continue that scientific work which alone qualified him for membership. What are his chances of success?

Suppose we had in this country a young William Harvey and a young Réné Theophile Laënnec. What would we do with them? We would certainly give them a good training in their medical schools and hospitals and we would provide them with fellowships or minor teaching positions which would allow them to devote most of their time to research work until they reached their early thirties. Then would begin the critical period. If they showed aptitude for executive work they would be given increasing responsibilities in the management of the clinic and medical school. If they showed aptitude for teaching they would have to spend more and more time with the students. If they were successful as clinicians they would have to look after not only their ward patients but also an increasing number of friends, nurses, and doctors' wives. They might in addition have private patients. Having already attained distinction in research they would be consulted by younger men seeking advice and many visitors seeking instruction.

At this stage Harvey and Laënnec would be put in charge of departments of medicine. With it would come an increased amount of paper work, budgets, schedules, and numerous committee meetings. I do not wish to discuss the question of full time versus part time, but it is quite possible that the university might stop their private practices which brought them in contact with instructive manifestations of disease in order that more time might be devoted to budgets and committee meetings.

From now on Harvey and Laënnec would be penalized for each new success in administrative, clinical or pedagogical ability. If they were really able administrators their departments would be increased in size and embellished with sub-departments until they were running an almost complete medical faculty with anatomists, physiologists, bacteriologists, etc., all under the department of medicine. If they were really distinguished clinicians great pressure would be brought to bear until they were taking care of many persons who had influence with the university. If they were public-spirited men desirous of advancing the cause of scientific medicine and public health they would be placed upon numberless committees in the county or national medical societies.

What would become of the discoveries that made Harvey the founder of modern physiology or the pathological studies that made Laënnec the founder of modern clinical medicine? How much time would be left for quiet work and contemplation?

You may answer that these men would have to take care of themselves and reserve a fair proportion of their time for their own work. How can a man best do this? A clever man would take care to make himself stupid at executive work and tardy at all committee meetings but what sensible man with the instincts of a doctor allows himself to be clever? A logical man would set aside a certain portion of each day and steel his heart against all interruptions; but what thoughtful doctor is ever logical with regard to his own time? Suppose he did reserve a morning for himself and in the midst of it a resident came to him saying that they were about to perform an autopsy on that interesting case with the big spleen, or an assistant blew in full of excitement with the result of his experimentum crucis or suppose there came with many bows that distinguished visitor from Germany or Japan.

The situation would not be serious if it only meant the loss of the original investigations of a few such men. There is a more serious loss; that is the gradual atrophy of the research spirit through disuse. The true research spirit can be maintained only by means

[1] President's address, American Society for Clinical Investigation, Atlantic City, May 2, 1927.

of personal contact and participation in the experiments. The head of the clinic is seldom allowed enough time for that study and work which alone will keep him the best informed man in his own particular field. The university scatters his energies over a broad expanse, part of which is as sterile as the Desert of Sahara. If we accept the recent definition of the specialist as the man who knows more and more about less and less we can define the chief of the large clinic as the man who is in danger of knowing less and less about more and more. Do we want our leaders in medicine to be transformed into deans or even college presidents?

The matter will become still more serious when the younger men begin to realize all this. It is quite conceivable that some of the most promising, those who are most devoted to research, may forsake the paths of academic medicine. They may sincerely believe that they can learn more medicine, support their families in more comfort and accomplish more in research if they devote half the day to private practice and the other half to the laboratory.

What remedies are possible? One makeshift remedy would be to have the men in charge of clinics go to other institutions for a certain portion of each year in order to segregate themselves from domestic cares. This would be a frank acknowledgment of the ineffectiveness of the home conditions. The only logical method would seem to be a diminution of the executive work thrown on one man's shoulders. The problem of simplification must be handled by the men in charge of our medical schools. They must realize that the head of a department can not always protect himself but that they must protect him just as he protects his younger associates. The prime case of the trouble is the desire for bigness on the part of our universities. Our great American sins are efficiency and bigness. About the time we achieve one hundred per cent. efficiency we succeed in eliminating everything except the empty shell of efficiency itself, and about the time we achieve the desired bigness we eliminate the last trace of greatness.

This still leaves us with the problem of what to do with Harvey and Laënnec. I am afraid that these worthies would settle the question themselves. After a good look at the present conditions they would realize that they would be happier in their own small inefficient but comfortable centuries. Harvey would beg to be returned to the England of James the First, Laënnec to the France of Louis the Eighteenth.

EUGENE F. DUBOIS

DEPARTMENT OF MEDICINE,
CORNELL UNIVERSITY MEDICAL COLLEGE

CHARLES WESLEY HARGITT

CHARLES WESLEY HARGITT was born at Lawrenceburg, Indiana, on March 28, 1862, and died at Syracuse on June 11, 1927. He graduated from Moores Hill in 1877, took his Ph.D. at Ohio University, 1890, and was awarded an honorary degree doctorate of science by Moores Hill in 1908, by Evansville College in 1920 and by Syracuse University in 1922. Dr. Hargitt was professor of natural sciences at Moores Hill College from 1885 to 1888; professor of biology and geology at Miami University from 1888 to 1901; professor of biology from 1891 to 1921 in Syracuse University; professor of embryology in the college of medicine from 1898 to 1912, and research professor of zoology from 1921. Dr. Hargitt was assistant director of the Cold Spring Harbor Biological Laboratory from 1891 to 1903 and a trustee of the Marine Biological Laboratory at Woods Hole from 1900 to 1921. When Professor Hargitt came to Syracuse, he immediately set about introducing the laboratory method into the introductory biology, and this course soon became one of the strong courses in the university. During these early years he taught geology and botany, as well as courses in geology, and later that in botany was placed in separate departments. Although he looked after these related subjects at first, he gave without assistance courses in general biology, invertebrate zoology, vertebrate zoology, histology and embryology. Seminar work was also included.

Professor Hargitt took an active part in educational organization, and early in his residence at Syracuse began urging a more liberal curriculum and in 1895 was instrumental in introducing the major and minor system, thus replacing the required courses of the last two years. He continued to be one of the leaders in faculty meetings up to the time of his retirement from active teaching.

But this diversified teaching and active part in educational reform did not hinder him from carrying on his research. Professor E. B. Wilson started him out as a graduate student on coelenterate development in 1884, and he continued to do research in this group for forty-three years. His contributions are too numerous to list, as they constitute a large part of the seven volumes of proceedings from the Department of Zoology.

Professor Hargitt studied at the Naples Biological Station in 1894, 1903 and 1910. He was vice-president in 1902 of section F of the American Association for the Advancement of Science; president of the New York State Science Teachers Association, 1898, and of Syracuse Academy of Science, 1896; member of the American Society of Zoologists; the American

Society of Naturalists; the Washington Academy of Sciences, and of other similar organizations.

Professor Hargitt was both a teacher and a friend to his many students. He was never content to admit that there was any difficulty in harmonizing the work of science and religion, and was ever ready to discuss these problems with students. We think of him as one of the pioneers in the teaching of the sciences at Syracuse. He has left a noble record of achievement and was fortunately able to carry on his research up to the end.

W. M. SMALLWOOD

SCIENTIFIC EVENTS

AN INSTITUTE OF PHYSICO-CHEMICAL BIOLOGY IN PARIS

BARON EDMOND DE ROTHSCHILD, who is a member of the Institute of France, in the section of "*membres libres,*" has made a gift of 30,000,000 francs ($1,200,000) to found and support an institute of physico-chemical biology, the need of which he suggested a number of years ago. The Paris correspondent of the *Journal* of the American Medical Association writes:

The proposed institute was already represented by a committee on organization. M. Girard, laboratory director at the École des hautes-études, will be the administrator of the new institute, which will be under the supervision of a board composed of the organizing committee, together with a number of distinguished scientists. The institute will be erected in the school quarter, in the vicinity of the Institute Curie. An institute of mathematical physics is likewise planned, funds for which will be contributed by the same donor. The text of the document by which M. Edmond de Rothschild conveys his gift is peculiar. Its tenor is inspired by the author's personal views in regard to biology and is intended to assure the continuance of the work of Claude Bernard, who was a friend of de Rothschild. It takes account, also, of Bernard's doctrine of physico-chemical determinism in relation to the phenomena of life. De Rothschild outlines thus, in a general way, the paths to be followed by the scientists of the future institute, and excludes, in advance, microbiology from their field of research. While the good intentions of the donor have been universally applauded, the restrictions that are thus placed on scientific research have been questioned in some quarters. The object seems to have been to found, over against the Pasteur Institute (the center of microbiology), a research center for the study of biologic phenomena, together with their practical applications, solely from the physical and chemical point of view. The gift is the largest that has ever been made in France by a single individual for the creation of an institute of pure science.

THE BARTOL RESEARCH FOUNDATION

DECISION to remove the research laboratories of the Bartol Research Foundation of the Franklin Institute from their present location in Philadelphia to the campus of Swarthmore College has been made.

It was only recently announced that Dr. W. F. G. Swann, who for the last two years has been the head of the department of physics at Yale University and director of the Sloane Laboratory there, would in September become director of the Franklin Institute Laboratories.

The Franklin Institute has leased a corner of the Swarthmore campus for its new building, but there has been no merger of the institute with the college.

Dr. Howard McClenahan, secretary of the institute, states that it is expected that the laboratories will profit by association with a college community, that they will have the conveniences of the college library and that they may be able to find students who can, at times, carry on researches of an elementary character by doing part-time work.

On the other hand, the authorities of Swarthmore College expect that the proximity of the laboratories and the associations of the research men in the laboratories will prove stimulating to the scientific departments of the college and will arouse interest among the students in research in physical science.

Mechanical and electrical disturbances due to city traffic have made it difficult to work in Philadelphia with the sensitive apparatus used in the laboratories and have resulted in the decision to move.

Nine men are now carrying on research work under fellowships of the Bartol Foundation, which maintains the laboratories, but the new building will accommodate about fifteen workers, in addition to the director and his assistants. It is expected that at least ten fellows will devote their full time to research work in different directions.

Dr. McClenahan pointed out that the plans for the laboratories were distinct from plans for a scientific, technical and industrial museum, which the Franklin Institute expects to build on the Parkway in Philadelphia.

PUBLIC BEQUESTS OF THE LATE PAYNE WHITNEY

THE residuary estate of the late Payne Whitney, who died on May 25, is by his will divided into 300 parts, of which about one third are bequeathed for public purposes. It is estimated that the value of these bequests is in excess of $20,000,000, and may be $50,000,000. The relevant provisions of the will are as follows:

(a) I give and bequeath unto The Society of the New York Hospital, twenty-two of said shares. I give and bequeath unto Cornell University, for the maintenance and support of Cornell University Medical College, five of said shares. I give and bequeath unto the New York

Public Library, Astor, Lenox and Tilden Foundations, eleven of said shares. I give and bequeath unto The Groton School, now located at Groton, Mass., two of said shares. I give and bequeath unto the president and fellows of Yale University, five of said shares. I give and bequeath unto the Nassau Hospital Association, one of said shares. It would be agreeable to me that each of the above legacies should constitute a part of the endowment fund of the institution receiving the same, the income to be used for its corporate purposes; but this expression of my wish shall not be taken to prevent the institution from applying any part of the capital of its legacy to the acquisition of additional real estate, buildings, equipment, facilities, furnishings or other property which its governing board may deem desirable for the development and extension of its public service.

(b) I give and bequeath unto the Society of the New York Hospital, to be applied to the development and maintenance of neurological or psychiatric work in New York City, eleven of said shares.

(c) I give and bequeath unto my son-in-law, Charles S. Payson, five of said shares.

(d) I give and bequeath the remaining forty-six of said shares unto Lewis Cass Ledyard, Lewis Cass Ledyard, Jr., and Edward W. Sheldon, in trust, to take possession, control and management thereof, and to invest and reinvest the same, and to collect and receive the rents, issues and profits thereof, and, from time to time, to use and apply the principal and income of said trust fund for the furtherance and development of such charitable, benevolent, hospital, infirmary, public educational, scientific, literary, library or research purposes as said trustees shall, in their absolute discretion, determine to be in the public interest. Such application may be made either by donations to institutions of the character above mentioned, or through the formation of corporations or associations for the development of such purposes, and contributions to such corporations or associations. It would be agreeable to me that this fund should be used for the aid or development of such of the above enumerated purposes as I have been interested in during my lifetime, but this expression of my wish shall not be taken to control or limit the absolute discretion above conferred upon the trustees of said fund.

EXPEDITIONS OF THE AMERICAN MUSEUM OF NATURAL HISTORY

AT a meeting of the board of trustees of the American Museum of Natural History, on June 9, arrangements were completed for a number of different expeditions. The expeditions as outlined were:

The Taylor-Sudan expedition, which will collect birds and mammals along the White and Blue Nile under Harold E. Anthony. Rollo H. Beck will direct the Whitney South Sea expedition to collect birds of the Pacific.

The Ruwenzroi-Kivu expedition, headed by Dr. James Chapin, DeWitt L. Sage and F. P. Matthews, will collect birds of subtropical Africa, and the sixth Bernheimer expedition will search for anthropological material in New Mexico and Arizona. George H. Tate and T. Donald Carter will start on a trip to Roraima, a mountain in Venezuela, where they will collect birds and mammals. Dr. Chester A. Reeds will conduct the Nevada meteorite expedition.

North Africa will be visited by George C. Valiant, assistant curator in Mexican archeology. A field expedition from the University of Chicago for an archeological reconnaissance of Asia Minor has been joined by Erich F. Schmidt, of the department of anthropology. The Putnam Baffin Bay expedition will visit Baffinland and other northern points under George Palmer Putnam.

The Trail-Side Museum and Nature Trails at Bear Mountain are under the direction of the resident naturalist, William R. Carr, and the insect station at Tuxedo is being continued by Dr. E. E. Lutz. Several expeditions are being arranged by Childs Frick for the department of vertebrate paleontology, among them being one to Nebraska under Albert Thomson and another to Colorado, Wyoming and Montana under Barnum Brown.

A resolution transferring the Aztec ruins at Aztec, N. M., to the United States government to form a part of the Aztec Ruin National Monument previously presented by Archer M. Huntington was adopted.

THE INSTITUTE OF CHEMISTRY AT THE PENNSYLVANIA STATE COLLEGE

TWENTY-TWO conferences on "Chemistry in World Affairs" will be held at the new Institute of Chemistry of the American Chemical Society, which begins its sessions July 4 at the Pennsylvania State College. The conferences, at which scientific men from both America and Europe will speak, will be similar to those at the Institute of Politics, Williamstown, Mass. Following each conference there will be a discussion by men prominent in the chemical industry and in education.

The chairman of the opening conference on July 5 will be Dr. Harrison E. Howe, of Washington, D. C., editor of *Industrial and Engineering Chemistry*. The principal speaker will be Dr. John E. Teeple, of New York, treasurer of the American Chemical Society. Dr. Teeple will speak on "Economic Factors in the Chemical Industry." His address will be discussed by Dr. Charles H. Herty, of New York, adviser to the Chemical Foundation, and William Haynes, of New York, editor of *Drug and Chemical Markets*.

"Economic Factors in Industrial Research" will be the topic of the July 6 conference, and "The Use of X-rays in Research on Structure of Non-Metallic Materials" that of July 7, when the speakers are Dr. Ancel St. John and Dr. W. P. Davey.

Other conferences and their speakers, so far as chosen, were announced as follows:

July 8, "Modern Research on Structure of Metals," Dr. Davey.

July 9, "The Control of Corrosion," William Blum, U. S. Bureau of Standards.

July 11, "The Control of Corrosion," W. B. Mitchell, Central Alloy Steel Corporation.

July 12, "The Training of Research Chemists."

July 13, "Training of Routine Plant Chemists and Continuation Education," Dr. R. E. Rose, E. I. du Pont de Nemours & Co.

July 14, "The General Theory of Catalysis," Professor E. K. Rideal, Cambridge University, England.

July 15, "Ammonia Synthesis."

July 16, "Ammonia Oxidation," G. B. Taylor, E. I. du Pont de Nemours & Co.

July 18, "High Pressure Synthesis," Professor H. S. Taylor, Princeton University.

July 19, "High Pressure Technique," F. A. Ernst, Fixed Nitrogen Research Laboratory, Washington.

July 20, "Modern Spectrum Analysis."

July 21, "Spectro Photometry," W. C. Holmes, U. S. Department of Agriculture.

July 22, "Chemical Microscopy," Professor E. M. Chamot, Cornell University.

July 23, "New Organic Solvents."

July 25, "Determination of Particle Size," S. E. Sheppard, Eastman Kodak Company, Rochester, N. Y.

July 26, "Oxidation-Reduction in the Living Cell," Dr. W. M. Clark, Hygienic Laboratory, U. S. Public Health Service.

July 27, "General Status and Problem of Nutrition," Professor H. C. Sherman, Columbia University.

July 28, "Vitamins," Professor J. C. Drummond, University College, London.

July 29, "Biochemistry."

The discussion group on July 14 will include Dr. Irving Langmuir, physical chemist of the General Electric Company, Schenectady, N. Y., Professor Taylor and Dr. C. H. Kunsman, of the Fixed Nitrogen Research Laboratory, and on July 13 Dr. C. M. Mackall, of George Washington University, and Dr. R. E. Bowman, of the Wilmington, Del., Trade School. This symposium will be presided over by Professor Neil E. Gordon, of the University of Maryland. Other discussion groups will be drawn from the universities and the industries.

Besides the conferences and the intensive courses, popular lectures on chemistry have been arranged under the direction of Professor Frank C. Whitmore, head of the department of chemistry in Northwestern University. Motion pictures will also be used to illustrate the progress of chemistry.

THE INTERNATIONAL SOIL CONGRESS

THE first International Congress of Soil Science is being held in Washington, D. C., from June 13 to 22, in the headquarters building of the Chamber of Commerce. Dr. Oswald Schreiner, senior biochemist in charge of soil-fertility investigations in the Bureau of Soils, is chairman of the executive committee of the congress.

President Coolidge was expected to open the congress. Secretary of Agriculture Jardine and Dr. A. F. Woods, director of the scientific work of this department, are to make addresses.

Abstracts of the papers have been prepared in English, German and French. These abstracts were distributed to the delegates on the first day of the congress so that they could have opportunity to familiarize themselves with the program in advance of meetings.

The program of the congress include local excursions about Washington, a reception at the Pan-American Union and a trip through the National Gallery of Art.

On the evening of June 22 the delegates will board a special train in Washington and start on a transcontinental tour which will cover a period of thirty days and will give the visitors an opportunity to study the soil regions and cropping conditions over a wide area of the United States and Canada.

In addition to the delegates from abroad, the experiment stations of the United States will be well represented by delegates appointed from their soils staffs. During the progress of the tour a number of the state experiment stations will be visited. Among these stations will be those of Tennessee, Georgia, Kansas, California, Oregon, Minnesota, Iowa, Indiana and perhaps a few others.

Dr. A. G. McCall, professor of geology and soils of the University of Maryland and in charge of soil investigations of the Maryland Experiment Station, is the executive secretary of the congress.

AWARD OF THE JOHN SCOTT MEDALS

AWARD of the John Scott Bronze Medal and $1,000 to each of six scientific men for "useful inventions for the use and benefit of mankind" was announced on June 3. The fund for the award was established in 1816 by John Scott, of Philadelphia. Winners of the award are:

Strickland L. Kneass, Philadelphia engineer, for the invention and development of an exhaust feed water heater injector.

Dr. Afranio Do Adaral, Bahia, Brazil, for his preparation of anti-snakebite serums.

Dr. Alfred Fabius Hess, professor of children's diseases at Columbia University, New York, for his studies on the influence of ultra-violet light on rickets and on various foods, showing that the vitamin values in these substances could be increased.

Herbert E. Ives, physicist, Bell Telephone Laboratories,

New York, for development of a method for transmitting pictures over telephone lines and for the further development of television either over land wires or by radio.

Dr. Peyton Rous, pathologist of the Rockefeller Institute, New York, for his invention of devices and the development of a filtration technique for separating from tumor cells a substance producing sarcoma in fowls.

Dewey DeForest Knowles, physicist, Western Electric Research Laboratories, Bloomfield, N. J., for a device known as a "Grid Low Relay," by which it is possible by an infinitesimal amount of energy to control the operation of an electric switch in an electric circuit capable of performing any desired operation.

SCIENTIFIC NOTES AND NEWS

PROFESSOR JOHN JOHNSTON, since 1919, Sterling professor of chemistry at Yale University, has resigned to join the United States Steel Corporation as director of its department of research and technology.

DR. WILLIAM CROCKER, director of the Boyce Thompson Institute for Plant Research, was recently elected chairman, and Dr. L. L. Woodruff, of Yale University, vice-chairman, of the Division of Biology and Agriculture of the National Research Council for the year 1927–28.

CORNELL COLLEGE has conferred the degree of doctor of laws on Herbert Hoover, secretary of the Department of Commerce.

THE degree of doctor of laws was conferred by Lafayette College on Dr. William S. Jardine, Secretary of Agriculture, at the commencement exercises on June 10. Dr. Jardine delivered the address, taking as his subject "Leadership in a Modern World."

DR. HUGH S. CUMMING, surgeon-general, United States Public Health Service, who spoke on the "Relation of the Physician to the Public" at the commencement exercises of the Medical College of Virginia, Richmond, was awarded the honorary degree of doctor of science. Dr. Cumming is an alumnus in medicine of both the University of Virginia and the Medical College of Virginia.

THE doctorate of science was conferred on Dr. Henry Fairfield Osborn, president of the American Museum of Natural History, on the occasion of the recent commencement of New York University.

DR. HARLOW SHAPLEY, director of the Harvard College Observatory, has been awarded the degree of doctor of laws by the University of Missouri, where he graduated in 1910.

THE University of Pittsburgh has conferred the honorary degree of doctor of science upon Mr. Andrey Avinoff, director of the Carnegie Museum, and Professor Hugo Kahl, curator of entomology in the museum.

AT the commencement exercises of the University of Maryland, the degree of doctor of science was conferred on Sir John Russell, director of the Rothamsted Experiment Station.

THE medal of honor of the Institute of Radio Engineers for 1927 has been awarded to Dr. Louis W. Austin, physicist of the laboratory for special radio transmission research of the Bureau of Standards. The presentation was made by Dr. Ralph Bowie, president of the institute, at the meeting on June 1.

DR. DOUGLAS JOHNSON, professor of physiography in Columbia University, has been elected a corresponding member of the Russian Geographical Society.

DR. SOLOMON SOLIS COHEN, professor of clinical medicine in the Jefferson Medical College and Hospital, has resigned his chair and has been made emeritus professor.

PROFESSOR C. F. JACKSON, head of the department of zoology in the University of New Hampshire, was at the recent annual meeting at Waterville elected president of the state academy of science to succeed Dr. Wilhelm Segerblom, head of the department of chemistry at Phillips Exeter Academy.

AT the recent meeting of the American Society of Mechanical Engineers at White Sulphur Springs, West Virginia, officers were nominated as follows: *President,* Alexander Dow, president of the Detroit Edison Company, Detroit, Mich.; *Vice-presidents,* John H. Lawrence, vice-president and engineering manager of Thomas E. Murray, Inc., New York, N. Y.; Newell Sanders, Newell Sanders Plow Company, Chattanooga, Tenn.; Edward A. Muller, vice-president and general manager, King Machine Tool Company, Cincinnati, O.; Paul Wright, Paul Wright and Company, Birmingham, Ala.

THE American Society of Agricultural Engineers has elected Colonel O. B. Zimmerman, experimental engineer with the International Harvester Company, president, to take office following the meeting at St. Paul, which will be held from June 22 to 25. Arthur Huntington, public relations engineer, Iowa Railway and Light Corporation, will be first vice-president; Deane G. Carter, professor of agricultural engineering of the University of Arkansas, second vice-president, and Raymond Olney, St. Joseph, Mich., secretary and treasurer.

DR. WILLIAM SNOW MILLER, professor emeritus of anatomy at the University of Wisconsin, was elected an honorary member of the National Tuberculosis Association at the Indianapolis meeting.

COLONEL LOUIS C. TRIMBLE, superintendent of the New York Post-Graduate Medical School and Hospital, who was last year chosen president-elect of the Hospital Association of the State of New York, presided at the third annual conference of the State Hospital Association, which recently met at Syracuse, N. Y.

ARNO C. FIELDNER, superintendent of the Pittsburgh experiment station of the United States Bureau of Mines, has been appointed to the position of chief engineer of the Division of Experiment Stations, effective on July 1.

W. G. CAMPBELL, director of regulatory work of the United States Department of Agriculture, will administer the work under the Food, Drug and Insecticide Administration, which takes form on July 1, 1927. Dr. P. B. Dunbar, now assistant chief of the Bureau of Chemistry, will be assistant chief of the new administrative unit, which was created by an act of the Congress and is charged with the enforcement of the federal food and drugs act, the tea inspection act, the insecticide and fungicide act, the naval stores act, the import milk act and the caustic poison act. Mr. Campbell has been connected with the work of enforcement of the food and drugs act since it became effective twenty years ago. Dr. J. K. Haywood, chairman of the Insecticide and Fungicide Board, which has been abolished, will have immediate charge of the work involved in the enforcement of the insecticide act.

S. A. ROHWER, entomologist in charge of taxonomic investigations in the Bureau of Entomology, has been appointed business manager of the bureau. He will continue his administrative work as head of the taxonomic division. E. B. O'Leary has been appointed administrative consultant, which position has recently been established. Herbert H. Schwardt, a graduate of Kansas Agricultural College, has been appointed junior entomologist, and will be associated with A. J. Ackerman in fruit-insect investigations at Bentonville, Ark. Lawrence C. McAlister, Jr., a graduate of South Carolina Agricultural College, has been appointed junior entomologist and assigned to duty at Riverton, N. J., where he will be associated with E. R. Van Leeuwen in codling-moth investigations.

MAJOR PETER BUCK, director of Maori hygiene under the New Zealand government, has been appointed director of Polynesian research by the Bishop Museum of Hawaii.

LESTER S. GRANT, dean of the faculty at the Colorado School of Mines, has been granted a leave of absence to accept the managership of an oil field in Texas.

CHARLES E. CHAMBLISS, associate agronomist of the U. S. Department of Agriculture, in charge of rice investigations, left Washington on May 3 for Cuba, on a trip to investigate the possibilities of growing rice as a commercial crop on some of the sugar plantations of the island. His trip is made under the auspices of the Tropical Plant Research Foundation.

DR. J. J. SINGER, associate professor of clinical medicine at Washington University Medical School, St. Louis, sailed for Europe on June 17, on a tour of inspection of chest clinics in the various medical schools of England and the continent.

DR. W. F. FOSHAG, of the Smithsonian Institution and the Harvard Mineralogical Museum, left on June 6 for Mexico, where he will make a survey of mineral deposits. Last year the Smithsonian and Harvard museums sent Dr. Foshag on an initial expedition to survey and make collections from Durango, Chihuahua and other states of Northern Mexico. His present trip is in continuation of this work.

DR. ALBERT SCHNEIDER, of the North Pacific College, will make another trip into the Mohave country for the purpose of continuing his studies of the pseudo-trachoma which is endemic in that region. He will also make observations of the results following gland operations according to the method of Dr. L. L. Stanley, of the San Quentin Prison. He is continuing some tests with a high capacity ozone generator, cooperating with members of the college medical staff.

CARL O. ERLANSON, junior instructor in the department of botany of the University of Michigan, has been granted a fellowship by the American-Scandinavian Foundation for the academic year 1927–1928. Mr. Erlanson accompanies the Hobbs Expedition to Greenland in the summer of 1927, and when that expedition leaves Greenland he will proceed to Disco Island, where he will spend the winter studying the collection of Arctic plants in the herbarium in charge of Professor Porsild at that station. In the early summer of 1928 Mr. Erlanson will go to Copenhagen and continue his study of Arctic plants in the herbaria.

DR. J. J. R. MACLEOD, professor of physiology in the University of Toronto, will be the lecturer next year under the Louis Clark Vanuxen Foundation at Princeton University.

SIR JOHN RUSSELL, director of the Rothamsted Experiment Station at Harpenden, England, gave three lectures at Cornell University during May on the

work of the station and scientific investigations in soil fertility.

THE South Dakota Academy of Science held its thirteenth annual meeting at Brookings on May 20 to 21. Sir John Russell, of the Rothamsted Experiment Station, gave two addresses as follows: "Eighty Years of Rothamsted Experiments" and "The Story of British Agriculture." Friday afternoon and Saturday forenoon were devoted to the reading of papers by the members.

DR. L. BOEZ, formerly assistant to Professor Calmette and assistant director of the Pasteur Institute, Paris, recently came to the University of Montreal to lecture for ten weeks at the new Institut Scientifique Franco-Canadienne. Dr. Boez is now professor of bacteriology at the University of Strasbourg and last year worked with the American Commission which went abroad to study foot and mouth disease.

DR. F. K. RICHTMYER, professor of physics at Cornell University, addressed the Sigma Xi Club of West Virginia University on May 20. Discussing the problem of radiation in particular, he emphasized the need for greater attention to the history of science.

PROFESSOR S. C. BROOKS, head of the department of physiology at Rutgers University, addressed the seminar at the Boyce Thompson Institute on "Electrical Conductivity of Bacteria" on May 12. At the same meeting Dr. M. M. Brooks, formerly of the Hygienic Laboratory, Washington, D. C., spoke on "The Penetration of Certain Oxidation-reduction Dyes into Valonia."

PROFESSOR H. R. KRUYT, who is to be guest of honor at the Fifth National Colloid Symposium to be held at the University of Michigan June 22, 23 and 24, arrived in this country about May 1. During the past month he has given a series of lectures in Washington and Detroit, at Johns Hopkins University, Massachusetts Institute of Technology, Cornell, Oberlin College, University of Wisconsin, University of Minnesota, University of Iowa, and at the University of Michigan. After the Colloid Symposium he will remain at the University of Michigan for a series of lectures in the summer session at that institution.

LECTURES given during the past season before the Sigma Xi Club of the University of Florida included the following: by Professor A. P. Black, of the University of Florida, on "The Synthesis of Phenyl Nitro Methane"; by Professor Robert Goodwin, on "Rubber"; by Dr. L. H. Pammel, Iowa State College, on "Fundamental Training in Relation to Scientific Research"; by Dr. L. R. Jones, of the University of Wisconsin, on "Some Problems concerning Plant Disease and Disease Control," and by Dr. W. A. Murrill, formerly of the New York Botanical Gardens, on "Mushrooms, Edible and Poisonous."

A SERVICE commemorative of the life and work of Professor Charles Sprague Sargent, Arnold professor of arboriculture at Harvard University since 1879 and director of the Arnold Arboretum for fifty-four years, was held on June 8. The speakers included President Lowell; William C. Endicott, trustee of the Massachusetts Horticultural Society; Mrs. John A. Stewart, Jr., president of the Garden Club of America; Ernest H. Wilson, president of the Horticultural Club of Boston and assistant to Professor Sargent for many years and now keeper of the Arboretum, and J. Horace McFarland, editor of the Rose Annual of Harrisburg, Pa. Roger Wolcott, until this year chairman of the committee to visit the Arnold Arboretum and an overseer of Harvard, presided. The Right Reverend William Lawrence pronounced the benediction.

THE thirty-second annual meeting of the Michigan Academy of Science, Arts and Letters was held in Ann Arbor, on April 27, 28 and 29. Nearly one hundred and fifty papers were presented before the various sections and general meetings. The new members elected number eighty-two. The following officers were elected: *President*, W. B. Pillsbury; *vice-president*, James H. Hanford; *secretary*, Lee R. Dice; *treasurer*, Robert B. Hall; *editor*, Peter Okkelberg; *librarian*, W. W. Bishop. The section chairmen chosen are: Anthropology, W. H. Worrell; Botany, Bradley M. Davis; Economics and Sociology, J. V. Van Sickle; Geography, Jerome Thomas; Geology and Mineralogy, S. G. Berquist; History and Political Science, Paul Cuncannon; Language and Literature, John G. Winter; Mathematics, Alfred L. Nelson; Psychology, C. H. Griffitts; Sanitary and Medical Science, Max Marshall; Zoology, Carl L. Hubbs.

ESTABLISHMENT of a permanent organization to be known as the International Fisheries Conservation Council of the Great Lakes was effected at a recent conference called to deal with the conservation of the fishing industry on the lakes, according to an announcement by Henry O'Malley, commissioner of the U. S. Bureau of Fisheries. The conference held on March 3, which was called by Governor Fred W. Green, of Michigan, was attended by representatives from the eight states bordering the lakes, and from the Province of Ontario, and by officials of the Bureau of Fisheries. To cope with the situation on the Great Lakes, the conference agreed upon recommendations regarding regulations and uniformity of action upon the part of the interested states. It was agreed also that wider powers should be accorded the fisheries authorities.

UNIVERSITY AND EDUCATIONAL NOTES

ACCORDING to the *Journal* of the American Medical Association, the Rockefeller Foundation will give $650,000 toward the construction of a new $1,500,000 building for the faculty of medicine at São Paulo. As a condition of the donation, the state government has agreed to construct hospitals in connection with the new medical school. The department of the interior of the state at São Paulo has received also from the Rockefeller Foundation $50,000 as the first instalment of a gift with which to construct an institute of hygiene.

A FUND of $500,000 to be given to Athens College, Greece, on the condition that Greeks in the United States supply an equal amount for building the college, has been raised, according to an announcement by Albert W. Staub, American director of the Near East Colleges.

THE contract for an addition to the chemistry building at the University of Wisconsin, to cost $390,000 with equipment, has been let.

BY action of the board of regents on May 4 the department of engineering graduate study and an engineering experiment station were established at Oregon State Agricultural College. Dean G. A. Covell, formerly dean of the school of engineering and mechanic arts, was appointed dean of engineering graduate study and director of the experiment station. Professor H. S. Rogers, formerly head of the hydraulic engineering department, was appointed dean of the school of engineering and mechanic arts and professor of civil engineering. Professor S. H. Graf, head of the department of mechanics and materials, was appointed associate director of the engineering experiment station.

THE *Journal* of the American Medical Association reports that at the annual meeting of the North Carolina State Medical Society Dr. Wilburt C. Davison, of Baltimore, outlined plans for the proposed Duke University School of Medicine at Durham. Dr. Davison was recently appointed dean of the new school of medicine. It is planned, he said, to limit the classes to fifty, and to arrange the curriculum so that, with proper preparatory education, the course leading to a degree in medicine should be completed in three years. The teaching and hospital staff will be full-time instructors, amply compensated for their services. Construction will begin soon on a 350-bed hospital and the school of medicine is expected to open in 1929.

DR. WILLIAM CHARLES BAUER, head of the department of electrical engineering, who has been acting director of Northwestern University School of Engineering for the past two years, since the death of the late John F. Hayford, has been chosen director of the school.

DR. CHARLES CLARENCE BIDWELL, professor of physics at Cornell University, has been appointed head of the department of physics at Lehigh University. Dr. Bidwell will succeed Professor Barry MacNutt, who has relinquished the administration of the department but will continue in his professorship.

DR. ERWIN A. ESPER, assistant professor of psychology at the University of Illinois, has been appointed associate professor of psychology at the University of Washington, Seattle.

ORLAND E. WHITE, curator of plant breeding at the Brooklyn Botanical Garden, has been appointed professor of agricultural biology and director of the Blandy experimental farm at the University of Virginia.

DR. C. A. EDWARDS, professor of metallurgy and acting principal of the University College of Swansea, has been appointed principal of the college in succession to Dr. Sibly.

PROFESSOR CARLOS CHAGAS, director of the Oswaldo Cruz Institute, has been nominated professor of tropical diseases at the faculty of medicine at Rio de Janeiro. He has been succeeded in the post of director of the Public Health Department of Brazil by Dr. Clementius Fraga, professor of clinical medicine.

DISCUSSION

SELECTIVE REFLECTION IN THE FAR ULTRA-VIOLET

FROM considerations bearing on the general theory of dispersion, the existence of resonance frequencies in the far ultra-violet has been predicted. Recently the writer has found the first of these for rock salt near λ 1590 A. U. The calculated value, according to Herzfeld and Wolff (Ann. d. Physik—25, p. 46—1925) is 1580 A. U. A vacuum spectrometer was used in connection with a specially designed vacuum tube by means of which it was possible to obtain spectrograms of the direct and reflected radiations. Of the other materials studied, crystalline quartz, calcite and glass showed an increasing reflecting power with decreasing wave-length. Speculum metal, on the other hand, showed the reverse. The work is being extended to the region of wave-lengths less than 1050 A. U.

A. H. PFUND

THE JOHNS HOPKINS UNIVERSITY,
JUNE 7, 1927

INFORMATION CONCERNING SUMMER PLANS OF AMERICAN MEN OF SCIENCE

MEMBERS of the American Association for the Advancement of Science who are to be away from their regular addresses for the summer, especially if they are to be engaged on scientific expeditions or missions or if they are to attend meetings of learned societies abroad, are asked to inform the permanent secretary's office as to their plans, giving information by which they may be reached during their absence. If this request meets with the general response the permanent secretary's office may function as an exchange for information on the whereabouts of members during a period when they are sometimes difficult to locate. The permanent secretary hopes to inaugurate in this way a useful service to members and to the work of the association. The file of summer plans and addresses may serve as a basis for subsequent study regarding the summer doings of American workers in science. The Washington office is always glad to supply information to members as to the whereabouts of other members, as far as such information is at hand.

BURTON E. LIVINGSTON,
Permanent Secretary

THE QUANTITATIVE THEORY OF SEX

IN a note published in SCIENCE (Vol. 65, Number 1675) Dr. Riddle complains that the present writer when publishing a note on the quantitative theory of sex (SCIENCE, Vol. 64, p. 299) made no reference to Riddle's work, which also aims at a quantitative theory of sex. I am still unable to see where in the note in question I could possibly have mentioned Riddle's work, which in no point whatsoever has anything to do with the quantitative relation or balance of sex-genes, with which I was dealing. As far as I can see the only point in common between what I call—rightly or wrongly—my quantitative theory of sex and Riddle's views about metabolic rate and sex is the word "quantitative." Riddle wants me to write "*A*" instead of "*The*" quantitative theory; I am gladly willing to do so. But his following remarks can not pass without comment. According to Riddle my work is "unimportant" for *the* quantitative theory of sex, as developed by him. "This [*i.e.*, Riddle's] particular theory rests essentially on the demonstrated fact that the entire normal genetic equipment (or the chromosomal determiners) for femaleness may, under experiment, be made to produce a male and *vice versa*; and that intermediate stages of sexuality may be thus produced." I had always believed that this was exactly what I have accomplished, the production of all stages of intersexuality between males and females and *vice versa*; and sex reversal in both directions within the genetic constitution of the original sex. I had also always believed that my theory was first derived from and for the explanation of these experiments. Now I am informed that my work has only to do with normal sex-determination, the facts of which form, according to Riddle, only an unimportant part of the present question. In the passage in question Riddle prints *normal* factorial basis in italics. This might possibly mean that in my experiments the factorial basis was not normal on account of crossing. But this can hardly be the case, as some of Riddle's own main arguments are based on work in hybridizing doves. Thus I am completely at a loss to understand why the facts of first importance, according to Riddle, are "indeed unimportant," when they happen to be found by myself.

But, since Riddle has now raised the point, I may be permitted to explain why the present writer (like most geneticists) has always been unable to accept Riddle's contentions as proven facts. I am not discussing now the recent work of many writers on actual sex-reversal in birds and toads, but the old work of Riddle on which his contentions are based. Riddle has stated over and over that he produced experimentally all stages of intersexuality and further sex-reversal. After repeated search among his papers I have completely failed to find any proof of the production of intersexuality. The only facts which I could find were the claim that after crossing of pigeons females were produced, which besides being morphologically normal and behaving normally towards males, showed abnormal mating instincts towards females, namely, acting in different degrees like males. Let us compare this with the work in moths, where intersexes were experimentally produced which in every organ of their body, including the gonads, showed all transitions from one sex to the other. As long as Riddle can not produce any other evidence of intersexuality than these mating instincts, no geneticist will accept his claim of experimental production of intersexuality.

His second claim is the experimental production of sex-reversal by reproductive overwork and by crossing. This contention is based on the assumption that the first egg of a clutch in pigeons is male, the second female. In family crosses the otherwise normal female produces only male offspring; generic crosses produce male offspring in spring, after overwork in autumn only females. From these and similar data Riddle draws the conclusion that sex-

reversal within the egg has been accomplished. Further evidence for this is given in a number of tests, mostly dealing with egg-size and stored energy. As I have recently ("Ergebnisse der Biologie," II, 1927) discussed these things I do not want to repeat the details here. It is simply a fact that no genetical or embryological proofs of sex-reversal have been furnished in this case (both have been amply furnished in my work with moths), that all evidence is circumstantial and in addition based on assumptions of the nature of a *petitio principii*, as shown in my recent discussion. With all due respect for Riddle's physiological and biochemical work involved in the study of his case, I must state that he has never produced (or never published) experimental intersexuality and never proved experimental sex-reversal or made it even probable.

Has then Riddle's theory of sex determination by different metabolic rates within the eggs in spite of its poor foundation, helped in any way towards an understanding of sex-phenomena? I am unable to see such a success. The theory already fails in the normal case of male heterogamety; it fails in such cases of female heterogamety as the gipsy moth, where the same egg can be made to develop into a female, a male, a female intersex and a male intersex if fertilized with the proper sperm; it fails in such cases as Drosophila where a normal egg gives a male with a Y-sperm, but an XX egg a female with the same sperm. Of course, Riddle might say that in all these cases the metabolic rate has been changed. But we want to know why it has been changed. There is nothing new in the idea that in the last resort sex, like everything else, is the outcome of chemical processes, which might also be called metabolic. To measure metabolic processes in the two sexes is certainly very meritorious work; but in my opinion it has not led a single step towards an understanding of the problems of sex-determination, although the methods used are quantitative in nature.

R. GOLDSCHMIDT

BERLIN-DAHLEM

THE ORIGIN AND ANTIQUITY OF MAN; A CORRECTION

MY colleague, Aleš Hrdlička, in a letter dated May 23, 1927, has courteously called my attention to two or three serious errors in my article entitled "Recent Discoveries relating to the Origin and Antiquity of Man," which appeared in SCIENCE on May 20. I deeply regret that these errors and omissions should have been overlooked in correcting the proof and I find they are not due to mistakes of the printer.

On page 484, right column, last sentence: "We consequently reach an entirely new estimate of the brain capacity of the human race at the close of Pliocene time and the beginning of Pleistocene time, a period estimated at between 1,250,000 to 1,600,000 (not 1,000,000,000) years before our era."

On page 484, right column: The statement, "This Dawn Man has a flat vertical forehead like the modern Bushman. . . " is correct.

On page 485, left column, and on page 488, left column: The tables should have had the word 'Minimum' prefixed to the measurements of existing native and European races; as it is, the figures give a false impression. The three races especially concerned in these tables are the 'native Indian Veddahs,' the 'living broad-head race of Czechoslovakia,' and the 'average modern Swiss.' The mean, minimum and maximum, male and female ccm. brain capacity of these races is as follows:

	Male			Female		
	Mean	Min.	Max.	Mean	Min.	Max.
Native Indian Veddahs	1250	1012	1408	1139	1037	1217
Living broad-head race of Czechoslovakia	1415	1230	1800	1266	1000	1400
Average modern Swiss	1467	1200	1660	1349	1230	1510

In the above measurements in my article of May 20, assembled by my colleague, Dr. H. L. Shapiro, it will be observed that comparison was made between the skull capacity of the Stone Age races and the *minimum* capacity of existing races, namely the 'native Indian Veddahs,' min. male 1012, min. female 1037; the 'living broad-head race of Czechoslovakia,' min. male 1230, min. female 1000; the 'average modern Swiss,' min. male 1200, min. female 1230.

Dr. Hrdlička remarks, "it is a general law that the males of any people exceed the females in capacity by from 150 to 200 ccm." also, "Finally on page 488 left column you give the capacity of the living broad-head race of Czechoslovakia as 1230 ccm. for the males and 1000 ccm. for the females—which is quite incorrect, the capacity of these people, stature for stature, equaling and even exceeding (see Weisbach) that of other European nations." Unfortunately, we do not know the sex of either the Piltdown or Trinil races. In the Neanderthaloid races, the female Gibraltar brain is known to be inferior in capacity to the male Neanderthal.

Finally, the general contention of my article is sustained, namely that at the close of Pliocene and the beginning of Pleistocene time, a cube of brain capacity was attained not quite equaling the minimum existing capacity.

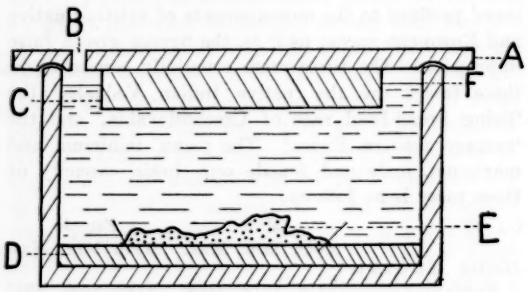
HENRY FAIRFIELD OSBORN

MAY 26, 1927

SCIENTIFIC APPARATUS AND LABORATORY METHODS

A CLEAR-VIEW SPECIMEN DISH

AFTER a number of experiments a museum dish has been evolved which can be used under the binocular microscope to good advantage. Valuable embryos and delicate dissections can be kept unharmed and viewed without optical distortion by this method. The accompanying figure gives the details of the dish.



A. Lid of stender dish.
B. Drilled hole for filling.
C. Plate glass disc cemented to A with Valspar.
D. Layer of wax.
E. Specimen attached to wax.
F. Upper surface of preserving fluid.

Clear glass imported stender dishes of various sizes were obtained. These had plate glass covers with a groove ground on the under surface. We next secured thick discs of clear polished plate glass (C in figure), somewhat smaller in diameter than the inner diameter of the stender dish. These discs were cut and polished at a local mirror and windshield factory. Such a disc (C) was then cemented to the under side of each lid (A). After many trials, an ideal cementing substance was found to be clear Valspar. The method for cementing was as follows: lid (A) is put with the upper surface down on a warming plate. A number of drops of Valspar was then put on the clean surface, and then the plate glass disc, perfectly clean, was placed upon the drops of Valspar, care being taken that no air bubbles were included and that a sufficient amount of Valspar was used to insure a small excess oozing out at the edges. A weight of about two hundred grams was then placed upon the plate glass disc and small pieces of lead plate were used as anchors to keep the glass from sliding while the varnish was still wet. The covers remained untouched for a number of days, the temperature of the warming plate never being over 60 degrees centigrade. It should be added that the lids of the dishes were previously bored at the glass factory with a hole (B), large enough to admit the tip of a small pipette.

A thin layer of beeswax, paraffin or ozokorite (D) was put into the bottom of each dish, and to this an embryo or dissection (E) was fastened by means of glass needles. The dish was then filled with formalin solution, alcohol or other preserving fluid, so that the upper surface of the liquid (F) was somewhat higher than the lower surface of the plate glass disc (C). Lid (A) was now cemented on to the dish by museum jar cement (Sealo), and the opening (B) plugged with a small stopper or a bit of beeswax. If there should be any subsequent loss of liquid from the dish, the plug can be removed and a small mouthed pipette used to replace the liquid through this opening.

The advantages of such a dish are evident. The air space is entirely out of the field of view, and there is no condensation of moisture on the under surface of the part of the lid used under the microscope.

S. I. KORNHAUSER

DEPARTMENT OF ANATOMY,
UNIVERSITY OF LOUISVILLE

A STARCH TEST IN PHOTOSYNTHESIS EXPERIMENTS

A SEARCH for better methods of conducting routine experiments in photosynthesis has led to results that may prove useful to others. So far as I am aware these exact methods have not been reported before.

We find that with a number of leaves such as those of the horseshoe geranium the chlorophyll may be dissolved much quicker by first rinsing or lightly rubbing them in gasoline. Kerosene seems to be somewhat useful but is not as good. While we have not determined the reason we find that hot alcohol penetrates many times as rapidly after this treatment, thus very quickly and evenly extracting the chlorophyll. The extreme brittleness from the alcohol treatment may be overcome by immersing the leaves for a moment in water.

With this method we find that ordinary white vaseline is excellent for coating the leaves instead of the various waxes commonly used to prevent the entrance of carbon dioxide in certain experiments. The vaseline is quickly dissolved by the gasoline treatment, thus permitting rapid penetration of the alcohol and iodine solutions.

A *fresh* solution of iodine in gasoline gives a clearer starch test than does the iodine solution in potassium iodide or in alcohol. It is, however, subject to a slow deterioration and becomes inactive after a week or two. It should be mixed in small quantities as needed.

To facilitate the use of this method a schedule is given below:

(1) Wash in gasoline one minute.
(2) Clear in hot alcohol until leaves are white. If needed change alcohol as soon as green with dissolved chlorophyll.
(3) Water momentarily to relieve brittleness.

(4) Iodine solution. A small crystal of iodine in 30 cc to 100 cc of gasoline.

FRED W. EMERSON

PENN COLLEGE,
OSKALOOSA, IOWA

SPECIAL ARTICLES

THE ROLE OF KINOPLASM IN THE GENESIS OF VACUOLES

THE genesis of vacuoles is a subject which is receiving much attention at the present time. Nassanov, Ludford, Uhlenhuth and Bowen, for example, have studied the development of secretory vacuoles in *Protozoa* and in gland cells of higher animals, and a host of workers, as exemplified by recent letters to *Nature*, are busy with the origin during oogenesis of various kinds of yolks—all of which, since they lie in pockets in the cytoplasm, are probably definable as vacuoles or more strictly the content of vacuoles—while Guillermond and his school and recently Bowen have investigated the inception of the "vacuome" in plant cells. It is argued by many, though Bowen disagrees, that the contractile vacuole of the *Protozoa*, and also the plant vacuole, are homologous with the Golgi apparatus of metazoan cells; that fatty yolk at least as well as certain secretory vacuoles are a product of the same structure; but that other kinds of yolk and of secretory vacuole may arise from mitochondria and from nuclear extrusions. Whatever rôle these various structures may play in secretion of the vacuolar contents, our own observations on living cells, chiefly of plants, lead us to the conclusion that the actual formation of the vacuoles is the function of a more active labile and fundamental element of the cytoplasm of which one rarely hears mention at the present time, *viz.*, Strasburger's *kinoplasm* (active substance). Recent cytological research has concentrated on those bodies which give more or less characteristic staining reactions, but the above differentiation of the cytoplasm is also sufficiently characteristic in another way, *viz.*, in the behavior in life, to warrant, it would seem, the continued application of a distinctive title.

The kinoplasm is distinguished from the matrix by a slightly higher refractive index, by its origin and metamorphoses, and usually by its motility—characteristically, protoplasmic streaming and the translocation of other formed bodies in the cell is a function of the kinoplasm alone. The form most commonly assumed is filamentous, but Strasburger recognized the identity of the fibrillar with the film type of structure and his conception is supported by our own observations (1) of the origin of the fibrils from and (2) their retransformation into films or membranes. This relation, which bears on the subject of our title, is illustrated in more ways than one. Thus the kinoplasmic processes which lie within the cytoplasm, *e. g.*, in cells of *Spirogyra*, may frequently be observed to stream out from the demonstrably differentiated film of clear cytoplasm which envelopes the chloroplasts, or they may become attached to such a surface at both ends flowing out from one and into the other. Similarly, they may arise from the boundary of the nucleus where also a differentiated film of cytoplasmic substance can be shown to exist. The probable origin of other intracytoplasmic processes from the external or the vacuolar membrane is only inferred from what follows.

Similar in behavior and origin to the above are the little known pseudopodium-like processes which commonly fringe the central sap cavity in plant cells. Here again by extending across the vacuole the filaments may flow both into and out of a differentiated film, the vacuolar membrane or tonoplast. While there is, no doubt, some difference in character between films and filaments which are in contact with the cell sap and those which are internal to the protoplasm, their similarity of behavior seems to allow of the extension of the generic term kinoplasm to cover both. (Of course the more massive strands which cross the vacuole may contain several streams of kinoplasm with matrix between.) We may, with Strasburger, include also cilia and fine pseudopodial extensions of the plasmatic membrane on the exterior of naked cells, but to extend the term as he did to nuclear elements (hypothetical at that) is to destroy its usefulness.

All this, however, is somewhat aside from our main point, which is that the typical kinoplasm within the cytoplasm becomes transformed on occasion into vacuoles or at least into the lining membranes of such. For example, during conjugation in *Spirogyra* the gametes get rid of water by means of "contractile" vacuoles.[1] These first become conspicuous as mucilaginous looking globules which in turn are formed by condensation of the kinoplasm. A similar formation of vacuoles—which also may be contractile, *i. e.*, may burst—frequently takes place under more pathological conditions, as for example, when cells are acted on by a strong plasmolysing agent which penetrates somewhat, or by a narcotic. In such cases the kinoplasm may give rise to vacuoles by other mechanisms than preliminary sphere formation. The erstwhile streaming processes may fuse into a honeycomb structure, or the various membranes, the vacuolar and chloroplastic envelopes particularly, may develop blisters in their substance.

In suggesting that these observations on the origin of vacuoles be taken into account in the study of yolk

[1] Lloyd, Trans. Roy. Can. Inst. 15 (2) 1926.

formation and glandular secretion, we are speaking with little direct knowledge but are influenced by several suggestive facts bearing on the relation of the kinoplasm to the other structures involved. As regards the Golgi bodies, the structure which reduces osmic acid is associated with a chromophobic substance, the "idiosome," which is usually recognized only in form of spheres, (also called archoplasmic, *i. e.*, kinoplasmic spheres) such as we have observed to swell into secretory vacuoles in *Spirogyra*. Gatenby's difficulty[2] in reconciling the sphere and vacuolar (Parat's theory) origin of fatty yolk seems to disappear in view of this observation. From certain drawings (*e. g.*, Bowen Q. J. M. S. 70, part 3) this chromophobic substance appears sometimes to be spread out over the Golgi apparatus. Similarly, the reducing substance in the tonoplast of plant cells is separable experimentally from the true membrane, which, like the kinoplasm generally, is at most only slightly osmophile. While the chromophilic Golgi substance may, as Bowen argues, synthesize and secrete certain vacuolar contents, we submit that the chromophobic substance which actually forms the vacuole is identical with the kinoplasm.

As regards the mitochondria, the kinoplasm is so intimately associated with this more conspicuous element that we wonder if operations have not been ascribed to the latter which really belong to the former in cells where the kinoplasm is less easy to observe than in plan cells. The relation between these two elements of the cell is shown in various ways. (1) The most abundant development of kinoplasmic processes is found in those cells and regions of a cell where mitochondria are densest. (2) The majority of the mitochondria are included in or attached to the kinoplasm. (3) A reciprocal relation is indicated by the observation that as the mitochondria are caused to increase by vital staining the active kinoplasm decreases in amount. (4) In its film form the kinoplasm occasionally stains lightly during the life by basic dyes. (5) The mitochondria are recognized as largely composed of lecithin and the behaviors of the kinoplasm are paralleled in many ways by those of lecithin and of lecithin-containing substances. Thus the kinoplasmic processes may be compared to the myelin growths of lecithin in water, while the vacuolization of kinoplasmic spheres in the cytoplasm—as also of lipoid containing droplets which may be caused to condense in the sap of many plants—is paralleled by the vacuolization and swelling of droplets of egg yolk (*e. g.*, of the cockroach) when squeezed out into water.

Our hypothesis, based on the above, as to the essential nature of the kinoplasm is that its differentiation from the cytoplasmic matrix depends on its larger lipoid content, resulting in orientation of molecules or micellae, in film formation, etc.; while the mitochondria we are inclined to regard as a reserve of substance for kinoplasm formation rather than the active elements they are claimed to be.

To sum up, while our observations tell us nothing of the process by which water and other materials are secreted in vacuoles, they certainly point to the formation of the enveloping films as a metamorphosis of the neglected kinoplasm and indicate that water at least may accumulate without the visible interaction of any other structure.

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[2] *Nature*, Dec. 11, 1926.

THE VIRGINIA ACADEMY OF SCIENCE

THE Virginia Academy of Science held its fourth annual meeting at the Virginia Polytechnic Institute at Blacksburg on May 6 and at the State Teachers College at East Radford on May 7.

The following officers were elected for the coming year:

Dr. Donald W. Davis, president.
Dr. L. G. Hoxton, member of the council.
Dr. Sidney S. Negus, publicity chairman.

Twenty-three papers were read before the section of astronomy, mathematics, and physics; twenty-nine before the section of biology; thirteen before the section of psychology and education; and twelve before the Virginia section of the American Chemical Society which functions as a section of the academy.

A prize of fifty dollars in gold offered by a friend of the Academy for a particularly meritorious paper read at the meeting was awarded to Dr. C. C. Speidel, of the University of Virginia, for a paper entitled "Regenerative Phenomena under Conditions of Hyperthyroidism." This prize was awarded by the permanent committee on the encouragement of research in Virginia which was established last year. President Horsley reported that he had succeeded in raising more than eight thousand dollars toward an endowment fund for the encouragement of research in Virginia, the income from which will be administered by this committee. A committee was also appointed to effect the incorporation of the academy.

After the meeting about eighty of the biologists made a field trip to Mountain Lake and about twenty of the chemists visited some of the industries at Pulaski.

E. C. L. MILLER,
Secretary-Treasurer